

REVIEWS AND COMMENTS

With the intent of stimulating discussion, this section is reserved for book reviews, comments, and letters; your input is welcome. By nature, this material may be subjective, reflecting the opinions of the authors; your responses are therefore encouraged.

Theory of Moves. By Steven J. Brams, Cambridge Univ. Press, Cambridge/New York/Melbourne, 1994. 248 pp.

Who is TOM?

Theory of Moves (“TOM”) is a new approach to the analysis of situations of conflict. In very bold strokes, it can be described as follows: consider a normal-form game—let us call it the “state game”—and imagine that it is played over time, where at each stage each player has a selected strategy. Assuming an initial state and a certain order of moves, TOM analyzes the extensive-form game generated by “unilateral deviations” from the current state. Let us refer to this extensive-form game as the “moves game.” TOM considers only ordinal state games, i.e., only ordinal ranking of outcomes is assumed to be given for each player; further, it assumes that only pure strategies can be chosen in the state game at each stage. The moves game is assumed to terminate by certain rules that make it finite. Thus one may apply the backward-induction solution to the moves game, and this is the basis for TOM’s analysis of the state game.

Who likes TOM?

TOM is likely to be highly controversial. On the one hand it appears rather intuitive and even compelling. On the other it will probably be rejected by many as too simplistic. The ordinal nature of the theory, the absence of probabilistic beliefs in it, as well as the lack of axiomatic foundations for TOM’s major assumptions will undoubtedly draw fire from the ranks of traditional game theorists. I would risk a guess that most of them

would like to see an analysis of the state game which is based on some sort of a Bayesian equilibrium among expected utility maximizing players who entertain beliefs about the game, about each other's beliefs, and so forth¹. At any rate, it seems that whether or not one likes TOM depends mostly on what one happens to believe game theory ought to be doing today. In order to explain why I like TOM, I have to digress and say a few words about this question.

The Market for Insights and the Public Lecture Test

What is game theory's trade? What does it produce and how does it make a living? Many would agree that the main contributions that game theory has made, and the primary export commodities it is likely to have in the future, are insights, that highlight certain aspects of a problem rather than describe it fully and accurately. Game theory offers explanations, predictions, and recommendations, but these tend to be qualitative rather than quantitative, tentative suggestions rather than definitive answers.

To the question, "What has game theory done for us lately?" my answer is, "Quite a lot." But I think that most of the benefit generated by game-theoretic research is in those conceptualizations and insights that can also be conveyed by very simple examples. Most of them, I would speculate, would pass the "Public Lecture Test": they can be explained to a general audience in such a way that (i) the listeners understand it and (ii) they feel that they learn something both new and useful.²

Are We Doing Our Best?

There is no denial that game theory has produced many such insights. Indeed, the recent book of Dixit and Nalebuff (1991) is a conclusive proof of that. However, the question is not whether game theory is producing something useful at all. Rather, we should ask whether it is doing so in an optimal way. Or, to be precise, whether we strike the right balance in our research efforts between mathematically and/or philosophically exciting theory and useful, enlightening examples.

¹ It does not take much courage to risk such a guess. Game theorists who think otherwise are probably not "traditional" by definition.

² There are, of course, some exceptions, that is, game-theoretic analyses which are very useful yet too complicated to explain to a general audience. Such exceptions will often be found on the borderline between game theory and operations research or engineering. Yet these more applied areas are hardly the main focus of game-theoretic research as reflected, say, in this journal.

Too Much Beauty

I suspect that the answer is negative. Game theory as we know it today seems to be over-mathematized and over-philosophized for its nearly officially stated goals. To the extent that it should be part of the social (and perhaps also biological) sciences, and to the extent that it should be a foundation of, or at least a language for, economics, sociology, political science, and so forth, one can hardly suppress the feeling that too much of what is called “game theory” is motivated by the aesthetics of the analysis, or by the field’s internal discourse, and too little of it is motivated by the need to contribute to fellow subdisciplines.

I tend to believe that game theory can and should be more useful. I do not think that any particular paper should be rejected on the grounds of being “too theoretical”; nor do I consider myself—or the author of TOM, for that matter—free of the sins of aesthetics. But I do think we should attempt to change the emphasis of the field as a whole.

What Is Great About TOM

Having said that, it is easier to explain why I like TOM. First and foremost, it is simple, understandable, and intuitive, yet new. It is not a simple task to look at a 2-by-2 ordinal game and say something new about it that captures some real-world phenomenon. True, TOM does not treat the state game as if it were “the” game; it actually analyzes the moves game which introduces additional assumptions. But these assumptions are rather plausible, even if not “axiomatically” derived and at times somewhat arbitrary. The main point is that TOM can say something about real-life situations that is convincing, yet not a-priori obvious.

Second, TOM suggests a new way of thinking about strategic situations. It highlights their dynamic aspect both through its emphasis on the role of the initial state and through its analysis of “moves,” treating state-game strategies as temporary choices which determine payoffs in the short run yet are subject to change at any time. One of TOM’s main contributions is likely to be the language of “moves”: by suppressing the details of the timing of decisions, discounting of future payoffs, and so forth, TOM focuses on the ability each player has to unilaterally deviate from a presumed “steady state.” Contemplated moves and countermoves are very intuitive notions, yet they are likely to be clouded when represented by strategies in an all-encompassing extensive form game. By contrast, treating them as primitives renders much of the analysis more lucid.

Third, one should give TOM credit for what it does not do. It does not assume that players have probabilistic beliefs over some barely imaginable spaces of strategies, types, and hierarchical beliefs over the latter. It does not assume that such beliefs are shared by all players, let alone that off-

equilibrium-path beliefs can be described by a common prior. It does not assume that players are expected-utility maximizers, nor, more importantly, that their beliefs about other players' strategies are accurate. (Of course, the backward induction solution used by TOM may be described as assuming all of the above, but it can also be derived from much weaker assumptions as well.) In short, TOM makes only rather plausible assumptions. True, they may appear somewhat ad hoc at times, but ad hoc does not mean unrealistic. Given the choice between the implausible but axiomatically founded and the plausible but ad hoc, it appears that the latter deserves more attention, especially in light of the efforts invested in the former.

Nobody Is Perfect

Reading TOM, I found myself nodding approvingly much of the time and enjoying both analysis and style throughout the book. However, when Brams compares TOM to the "standard game theory" one feels an urge to challenge him to a duel, or maybe even a truel. The author is fully aware of the "standard game theorist" objections, and he refers to them at certain points. Yet the comparisons with the standard theory are hardly fair. By and large, the "standard" (typically Nash equilibrium) solution to the state game is compared to TOM's predictions, which are based, as mentioned above, on the moves game. There is no wonder, indeed, that the predictions are rather different because, as Brams notes, the games are different. But having admitted this point, it is not clear what insight is gained from comparing the Nash equilibrium of a static, one-shot game to TOM's NME (non-myopic equilibria) which analyze the dynamic interaction.

Furthermore, the very term "non-myopic equilibrium," coupled with the claim that TOM introduces a dynamic aspect that the standard theory lacks, is a little misleading. True, in the state game TOM's players think a few stages ahead and consider the replies other players would make to their moves. But, in terms of the moves game, they consider unilateral deviations just as in a Nash equilibrium. Indeed, NME's are computed by applying a backward-induction solution to some finite, extensive-form, perfect-information game (with no ties). As such, NME's *are* Nash (and even subgame-perfect) equilibria.

I tend to view TOM as a reduced-form model, rather than as a rebellion against game theory. All that it does can be done by the standard theory. To be precise, all that it does *is* done by the standard theory, coupled with TOM's behavioral assumptions. Furthermore, these assumptions can be translated to the standard notions of "utility" and "beliefs" should one wish to represent non-myopic equilibria as Bayesian-Nash equilibria of an "all-encompassing" game allowing all possible moves at all stages. That is, the assumptions which are partly incorporated into the definition of the

(finite, extensive-form) “moves game” can be reflected in players’ beliefs and utilities in such a way that TOM’s non-myopic equilibria will be Bayesian–Nash equilibria of the all-encompassing game.

Thus TOM’s advantage should not be presented as providing more accurate predictions; rather, it is in simplifying the analysis and providing a convenient framework for reasoning that would otherwise be awkward.

There Is More to TOM

Needless to write, the theory of moves is more sophisticated than a mere backward-induction analysis of the moves game. The book offers a collection of ideas pertaining to the application of TOM. The “anticipation game,” for instance, helps analyze situations in which the initial state is not well-defined; similarly, the notions of “moving power,” “order power,” and “threat power” enhance TOM’s predictive power by modeling additional features of the game having to do with the order of moves, differential abilities of the players to sustain a state or cycling among states, and so forth.

A critical reader may argue that these are ad-hoc additions and modifications which only attest to the fact that TOM is not as general as one would hope. A sympathetic reader, on the other hand, would rejoice at finding new concepts which enrich the game-theoretic vocabulary and further our understanding of conflict situations. As often is the case, both views contain a few grains of truth. We may feel that TOM does not derive the complete analysis from a single principle and continue our search for a unifying theory. Yet we should not reject the insights provided by TOM in its present form.

The BotTOM Line

I strongly recommend reading *Theory of Moves*. Apart from all the insights it offers, and apart from its fresh outlook on situations of conflict, it is an amusing and informative book. If you manage to ignore Brams’ references to the “standard theory,” you are bound to enjoy it.

REFERENCES

- DIXIT, A., AND NALEBUFF, B. (1991). *Thinking Strategically: The Competitive Edge in Business, Politics and Everyday Life*. New York: Random House.

Itzhak Gilboa

*J. L. Kellogg Graduate School of Management
Northwestern University
Evanston, IL 60208*